SCIENCE

NEW YORK, MAY 15, 1891.

SOME POSSIBLE MODIFICATIONS IN THE METHODS OF PROTECTING BUILDINGS FROM LIGHTNING. — DISCUSSION.[1]

[Continued from p. 255.]

MR. EDWARD P. THOMPSON: — I have listened with a great deal of interest to the matter presented in the paper just read. The alleged facts seem to agree with our ideas of electricity of low potential. Electricity occurs in thunder-storms, and the best thing to do is to get rid of it. One way is by conducting it away sufficiently rapidly by means of a conductor of very large surface capacity, as the conductivity of a metal as to static electricity depends upon the surface and not upon the sectional area. This principle is applied in the ordinary lightning-rod. As I understand the speaker, he proposes to provide a system whereby the electrical energy is not conducted away, but converted into heat. In view of the conduction principle having so often proved a failure, and the conversion principle having succeeded every time, according to the researches of the speaker, and since his theory agrees with well-known electrical principles, I think Mr. Hodges has presented matter well worthy of the consideration of the institute, and I, for one, can find no objection to his system as to correctness of principle. As to practical equipment, some incombustible non-conductor, such as asbestos, should be placed between the thin metallic strip and the structure to be protected, or else the melted metal may set fire to the building.

Dr. William E. Geyer: — It seems to me that the occurrence quoted here from Franklin tends to show that the ordinary theory of the lightning-rod is essentially true. The bell-wire, so far as it went in the occurrence here described, was a lightning-rod, and protected the building so far as that lightning-rod went. It was not heavy enough to carry the current, and it was for that reason dissipated, so that the dissipation was simply an accident. The mere dissipation, however, did not save that part of the building where the wire stopped and there was no good conductor: the building was without any lightning-rod, and was more damaged than where it had even a small rod.

Mr. Townsend Wolcott: — Professor Lodge's theory of the Leyden-jar discharge is that it is oscillatory under ordinary conditions, that is, where the coatings are connected with a good conductor. Now, if they are connected with a bad conductor, such as a wet string, Professor Lodge says that the discharge may be only in one direction, that is, the energy is all dissipated in a single discharge; whereas, if the conductor is good, there is little energy dissipated in getting from one coating to another. So far, Mr. Hodges' theory would seem to agree with Professor Lodge's, that if you can use up the energy of the electricity in destroying the conductor you will get rid of it more quickly than you would in any other way, and the lightning will have less effect outside of that. But there are some other points. Mr. Hodges says we do not attempt to make a good connection at the top with the dielectric. I do not exactly understand that. We do attempt to give it good connection with a conductor. If a cloud is charged, that is a charged conductor, and so long as the current is to come down to the earth, we try to get as good connection with it as we can, by putting points on the lightning-rod, for instance. A better way to do that would be to have a flame or something of that sort. As an experiment in drawing electricity from the air, a flame is better than a point. But, of course, it would not work in a thunder-storm.

As to the point which Dr. Geyer just mentioned, that Mr. Hodges' experiments support the ordinary theory of the lightning-rod, I think his reasoning does, to some extent, too, in regard to getting rid of the energy on the central core. Take the ordinary lightning-rod. The way it is intended to work is rather to prevent a disruptive discharge than it is to take care of one that has already occurred. We desire to equalize the difference of potential by drawing off the charge from the cloud before it gets to a dangerous limit. If we can do that, we do not have any disruptive discharge at all. It is just like a brush discharge, such as you get from a conductor with points on an electric machine. I think the fact is not questioned that lightning sometimes is discharged in that way, but not always. There is the trouble. I do not think that any one system of lightning-rods has proved successful. Sometimes a lightning-rod will take care of several discharges in a single storm, and that seems to be something which Mr. Hodges' lightning-rod would not do; because, after it had been dissipated by one discharge, I do not think, even if it could be put up in a few moments, that anybody would care to be monkeying around a conductor when there was lightning. Mr. Hodges, having asked us to clear our minds of the idea of conducting electricity, seems to go further than most of the modern theorists on electricity. I think Mr. Hodges, even if he does not use the idea of electricity, will admit that we want to make a metallic way entirely down to the earth. The case is somewhat analogous to the Leyden jar; that is, two conductors separated by a dielectric. Now, we want to bridge over the whole space of the dielectric, whether you use the idea of conductivity or not. So I don't think it makes much difference whether you use his dissipatable lightning-rod or a stout one.

Mr. Hodges: — I would like to bring the discussion back once more. In order to make the paper of some length, I gave some theory; but the fact as I have found it is this: I know how the books state that the ship "Jupiter" was saved from destruction in spite of her lightning-rod going to pieces. But take the fact without going to the books at all. What do the records show? I want to get a case where the conductor has gone to pieces, and where the ship has not been saved. I have not found such a case. Suppose the conductor is dissipated between two points [illustrating]. I found this to be true in every single case of a church-tower being struck where the wire runs from the bells to the clock. The wire goes, and the church-tower is saved between those two planes. Now, that is a matter of record. The ship "Jupiter" had a chain conductor, and it was dissipated; and the

[1] A paper, by N. D. C. Hodges, read at the fifty-sixth meeting of the American Institute of Electrical Engineers, New York, April 21.

books say that the ship was saved in spite of the conductor being dissipated. But that is a man's opinion. What I want to urge on the institute is simply this: that in every single case when a conductor goes to pieces every thing else is saved. Why and wherefore, I do not care. I gave some theoretical reasoning which seems to me more or less correct, but it is unimportant. I want to bring the discussion back to simply this matter of fact: Can you cite a case where the conductor has gone to pieces, and there has been any destruction to the building between practically two horizontal planes passing through the upper and lower ends of that dissipated conductor? You will find that there is damage above and damage below, very likely. But as I went on through the "Philosophical Transactions" I found one case there of a thunder-storm passing over a village (it was a century ago, or more), and the people were dependent on the church-clock for the time. In the morning they did not hear the clock strike. They went up in the church-tower to see what the matter was, and found the windows smashed just above the bell; found the wires running from the bell to the clock were gone, and that was all the damage done. To show how small a conductor, when dissipated, will save a house, I will cite the case of a palace in France in the early part of the century. The interior was heavily gilded. The people were sitting around on gilded sofas resting against the walls, — resting against thin gilded strips. A Fellow of the Royal Society visited the palace the day after it was struck by lightning, and looked over the ground. No one was killed. No damage was done to that palace as far as the gilding extended; that is, in the gilded rooms no damage was done except that the gilding disappeared: it was dissipated. When they got to the lower portions of the palace, where there was no gilding, things were smashed. But, as I say, I started out in this thing with an hypothesis which was to a certain extent wrong. I looked into the records to see what was recorded, not as a matter of opinion, but to find out what actually happened. And what actually happened was, so far as I was able to judge, that there was no case where a dissipated conductor failed to protect a building under these limitations which I state. Of course, above or below, damage did occur.

A Member: — From a practical standpoint, would you kindly tell us where you put that conductor on a house, and would you put more than one strip on a building?

Mr. Hodges: — Along the ridge-pole, down the eaves, down to the ground. I should avoid, at the lower end, making connection with any large masses of metal. The number of strips placed on a building would depend on the size.

I got a patent on this thing last year. I told a friend of mine that I was interested in the protection of buildings from lightning, and, a patent not being issued, I could not tell him much about it. The next day he met me and said, "Did you read in the *Post* the account of the lightning-storm in Jersey yesterday?" I said, "No." He said, "There was one case where a house was struck by lightning in Jersey and the rod was smashed, but the house was uninjured." I noted it down as another case. A man who was in my employ some years ago came to my office. I described this thing to him, and he said, "I have been there." He said when he was a boy he had a telegraph line running from his house to a neighbor's house. It was made of piano wire, and the lightning struck the roof somewhere there [illustrating], then followed along the metallic gutter to a point here. This piano wire ran down to the ground, and ran over here to the neighbor's house. At this point a little damage was done to it. The discharge followed along the conductor without doing any material damage, and there was no other damage to the house except that the wire was gone.

Mr. Wolcott: — Although I do not question that conductors work that way, we also find that they work the other way, according to the old theory, in very many instances. It certainly is a matter of record that conductors have sometimes carried off several discharges in the same thunder-storm, which a dissipatable conductor could not do unless you were able to put up a second one in the place immediately after the dissipation of the first.

A Member: — The Washington Monument is a pretty good lightning arrester. I was shown, a few weeks ago, by Professor Owens, where lightning had struck and knocked out big chunks of stone from the monument. He seemed to think that lightning followed the path of least electrical resistance, so he put up an additional wire and connected that with the new iron work of the monument, and he says he has not had any trouble since then with stones being knocked out.

Mr. E. P. Thompson: — I have not heard of any experiments being performed upon Mr. Hodges' proposed system. It may seem, perhaps, impossible to perform experiments with lightning in a laboratory, because of the inconvenience of waiting for a thunder-storm; but it can be done with the induction system, and possibly, therefore, some way may be thought of for testing Mr. Hodges' invention. About three years ago I tried some experiments in connection with a client, a well-known lightning-rod manufacturer, Capt. Hubbell, who has equipped government magazines. His new system was tried with considerable elaborateness in the Equitable Building during its repair, for the consideration of the Standard Oil Company, who met with great losses of oil-tanks, caused by lightning. An immense Leyden battery was charged with an electrical friction machine, and artificial lightning was thus generated. Small oil-tanks containing alcohol — more easily lighted by the spark than petroleum — were equipped, and by discharging the battery it was easily determined how many times out of a hundred the Captain's system was successful. Some experiment with Mr. Hodges' system would soon settle the question of effectiveness.

Mr. Charles Steinmetz: — To one point more I wish to draw attention. By using such an interrupted conductor of small cross-section, that is of comparatively high resistance, you are liable to change the whole nature of the lightning discharge. You change it from an oscillating discharge to a steady and continuous rush of current, from which you must expect quite different effects.

When, for instance, you discharge a condenser by a conductor of very low resistance, you get an oscillating discharge of an extraordinary high frequency. If you increase the resistance of the conductor, the number of oscillations of the discharge decreases, it runs down quicker, until at last you reach a value of resistance where only one wave of discharge current appears, that is, the discharge of the condenser becomes steady. Now, if we can make a lightning discharge steady, instead of oscillating, then we have first to expect that the electricity traverses the lightning-rod only once, slowly increasing in current strength and then decreasing again by going down to the ground; while in an oscillating discharge the current will rush to and fro through the conductor until its energy is consumed by the resistance of the lightning-rod, or by electro-magnetic radiation and re-radia-

tion from the induced currents produced by the oscillating discharge in neighboring conductors.

This, perhaps, may account for some of those phenomena mentioned to-night: that, when the lightning-rod is dissipated, that is when its resistance was very high in comparison with the quantity of electricity rushing through, there was a steady discharge and no harm was done; while, when it is an oscillating discharge, the slightest irregularity will cause the discharge to "jump the track," that is, to leave the lightning-rod, which is obstructed by the counter electro-motive force of self-induction, and to spark over to metal masses of larger condenser capacity: for what I consider as the most dangerous part of lightning discharges is not the enormous voltage of the discharge, nor the strong current rushing through the lightning-rod, but the electromagnetic field of force, which alternates with enormous frequency and reaches far out into space from the real path or centre of disruptive discharge, and thereby must cause inductive effects everywhere, which, as before said, cause not only the main discharge to spark over, but produce true secondary or induced lightning discharges. Hence I must be very much in favor of every arrangement which is able to change the oscillating discharge into a steady rush of current.

The resistance of the lightning-rod I consider as of subordinate importance only, except so far as carrying capacity is concerned: for of what use is a resistance as low as a few ohms, when the self-induction of the lightning-rod causes a spurious resistance of perhaps hundreds of thousands of ohms?

Mr. Hodges: — I would bring this discussion back once more to this matter of fact that I am interested in. The theory I do not care so much about. It may be interesting as mental gymnastics. I came here feeling quite sure that somebody would stand up and say, "I know a church or a house in this town or that town where the conductor was dissipated and yet damage was done on the same level." I have not found a case.

Dr. Geyer: — In a disruptive discharge, the length of the lightning-rod, it seems to me, is a very small part of the total path. I should imagine that any resistance the conductor would have would be such a small part of the total that it would not have much effect on the character of the discharge.

Mr. Steinmetz: — I believe I have been misunderstood in what I meant by the influence of the resistance upon the nature of the discharge. Indeed, the whole resistance of the lightning-circuit is so large that under any circumstances the resistance of the lightning-rod is imperceptibly small. But, as explained in my former remark, it is not the resistance proper, but the consumption of energy by the resistance, which causes the amplitude of the oscillating discharge to decrease slower or quicker until, for a very rapid consumption of energy by resistance, only one wave appears that is a steady or continuous current. This phenomenon is similar to a pendulum oscillating in a liquid: the greater the frictional resistance of the liquid, the quicker the amplitude of the pendulum motion decreases, until, at last, in a very tough liquid, the pendulum comes to rest without any oscillation at all — periodically. In such a way the resistance of the conductor, by consuming the energy of the electric discharge, could change the discharge from an oscillating to a continuous one, although the whole "resistance" has still about the same value, "infinite," if we were allowed to speak with the usual meaning of "resistance" of disruptive discharges, which we are not.

Mr. Birdsall: — I think Mr. Hodges has given us the most original idea on this lightning-rod question that has been put forward for some time. I also think that Mr. Steinmetz has hit the nail on the head in his explanation of it. It only shows us again what we do not know about the various phases of alternating currents. His theory also gives me a little uneasiness, because I have advised a number of friends who have built houses in the country to put in a metal lath, as I thought that, having plenty of metal around, if the house happened to be struck, it would go to the ground through this metal lath. Now, if any of those houses are struck, and that metal lath turns into gas, I think I shall emigrate.

Mr. Hodges: — That metal lath reminds me, — I wrote to Edward Atkinson about this. You know he is president of about the only insurance company in the country that cares about stopping fires; that is, reducing the amount of damage done. He wrote back that they had experience with lightning-rods, and that their experience was such that they had abolished them on all factories that were insured by the Manufacturers' Mutual Fire Insurance Company. Now, in the mills there is a considerable surface of metal; and they find, as is natural, that the discharge spreads itself probably over the surface of this metal. At any rate, the potential was so reduced as to very materially mitigate the effects. As Mr. Atkinson puts it, it spreads out over the surface of the machinery, so that no great damage is done. But they have taken off their old rods.

Mr. Wolcott: — There is one question I would like to ask in regard to that drawing on the board. If you do not say that no damage was done to the end of the building, in spite of the fact that the conductor was dissipated, why don't you have to say that no damage was done along the eaves, in spite of the fact that the conductor was not dissipated?

Mr. Hodges: — That is a fair question. A dissipated conductor may run horizontally any reasonable distance, and then run down; and when it goes to pieces the thing is saved. But when the conductor is not dissipated, there are any number of cases where the building is not saved.

Mr. Wolcott: — I can understand it, that a dissipating conductor would very often save the building, but, according to the accounts that have been cited, it does not seem to make any difference how little there is of that metal. There must be some limit. When it gets down where a little bit of gold-leaf is going to save a building, it looks rather improbable. If a little bit of metal being dissipated would save a building from a lightning discharge, then an ordinary lightning discharge would not be sufficient to dissipate some of these larger conductors which are dissipated.

Mr. Hodges: — I do not pretend to understand any thing about it. I have theorized upon it, but that is not important. It is only the fact, and the fact stands there until somebody gets up and shows a specific case where it does not work.

Mr. Birdsall: — I do not think that Mr. Wolcott can hold that argument, because he has not any data on the comparative energies of these various discharges of which we have record. We have a record of the damage done in the dissipation of the conductor, but we have no record of the foot-pounds of energy in the discharge.

Now, the discharge that burnt up the gold-leaf on the wall might have been a great deal smaller than some of the discharges which burned up the larger conductors. Then another point has been raised about the replacing of the conductor immediately after it was dissipated. This will never be necessary, it seems to me, for it is a recorded or alleged

fact that lightning never strikes twice in the same place. They say that in naval combats the safest place to put your head is through the hole that the cannon-ball has just come through; and if it did strike more than once the rods could be arranged on the principle of the multiple fuse, and a new one plugged in as fast as they dissipated.

Mr. Wolcott: — Mr. Birdsall has been facetious on this point, and I will try to be so, too. I have heard it stated that one reason why lightning does not strike in the same place twice is that the place is generally gone when the lightning has struck once. I certainly have read of several cases where the conductor has conducted several discharges to earth in the same storm. Now, with regard to gold-leaf discharge. That this charge was smaller, of course, may be true. But the fact that the discharge in each of these cases is just about suited to the size of the conductors would seem to show that there was some coincidence about the matter. If a dissipated conductor always stops the damage, or very nearly always, there is something more than coincidence about it. It seems to me that such an instance as that could not be more than a mere coincidence — that a discharge which was capable of doing considerable damage to the building where the conductor was not dissipated, should be all used up by dissipating a very small amount of metal, is not probable.

The President: — I will call the attention of the Institute to the fact that our usual time of adjournment has very long passed.

Mr. Hodges: — Ships have been struck a number of times in the same storm. If you can cite specific cases against me, all right. I have found, so far as I know, that a dissipatable conductor protects. Why, is another question that does not concern us. Why that gold-leaf protected we do not care. It did protect. There is no arguing against its being reasonable, that will set aside the fact. I thought over the matter, and have some theoretical considerations to show why it does protect, but those are not essential.

This is all I want to give at the present time. But I believe there is one other way of furnishing protection against lightning which has been ignored for a number of years. The facts have been staring us in the face. I think about the same time that Harris introduced his system of lightning-rods there was a modification made in the rigging of ships which has tended to mitigate the disastrous effects of lightning. The facts were well known long before Harris came into existence; but they were so thoroughly out of tune with all the science of that day that they were simply ignored; so that, in fact, in the report of the lightning-rod conference, there is only the title of one paper bearing on the subject. To find that paper I hunted through the Astor Library, and put one of their expert searchers to work there; and it was evidently considered of so little importance, that it had not been copied in any periodical. By going back further and further in the "Philosophical Transactions," I found the same facts reported of a most positive character, and I think they have a bearing on this apparent immunity of ships when they are supplied with good conductors. I am inclined to think that it is not the Harris conductor that has been doing good service entirely, but it is something else. But all that I would have said this evening, if it had not been necessary to present a paper of some length, was that a dissipatable conductor protects.

Mr. James Hamblet: — I understand the gentleman to say that a dissipatable conductor protects. I have in mind a very large building situated at the top of a hill, in a very exposed position. That building is constructed with a metal roof, entirely over the building, but having no lightning-rods. It has large iron pipes, six inches in diameter, to conduct water through the building down to the ground. That building has never been injured by lightning at all, but frequently trees around it on the hill have been destroyed by lightning. The lightning conductors of the building, which are these same iron pipes I have mentioned, have not been dissipated.

THE BROOKLYN INSTITUTE BIOLOGICAL LABORATORY.

THE location of this biological laboratory, at the head of Cold Spring harbor, Long Island, is one of the most favorable on the coast. The country around affords excellent hunting ground for every form of animal and vegetable life common to the climate. Just above the laboratory is a series of three fresh-water ponds, each fertile in its own peculiar forms of fresh-water life, and through which flows the water of Cold Spring Creek. Just below the laboratory is the harbor of Cold Spring, divided by a sandy neck into an inner and an outer basin. The inner basin is particularly rich in marine life, and the channel between the inner and outer basins has a varied and vigorous growth of algæ, mollusks, and echinoderms. The outer basin has rocky projections, shallow flats, banks and eel grass, sheltered pools, oyster-beds, and other conditions favorable for collection and study. The outer basin opens into Long Island Sound, whose coast is varied in character for twenty miles in either direction.

The main laboratory occupies the first floor of the New York State Fish Commission building, and is a room thirty-six feet wide and sixty-five feet long, provided with ample light from every side. It is furnished with laboratory tables, aquaria, hatching-troughs, glassware, and all the apparatus and appliances required for general biological work. Into the laboratory is conveyed a bountiful supply of the water of the Cold Springs for use in the aquaria and troughs. This water is as pure as a crystal, has the same low temperature throughout the year, and is the water used so successfully by the New York State Fish Commission in hatching and growing salmon, trout, and other food fishes. The laboratory is also supplied with an abundance of salt water, which is pumped up from the harbor into a brick reservoir, from which it runs to the laboratory.

The station is provided with three small row-boats and a naphtha launch, together with nets, trawls, and dredges, for use in collecting and dredging. Near the main laboratory is a photographic room, with a dark room and work room adjoining. Each student is provided with dissecting instruments, chemicals, and glassware, to be used in the dissection, preparation, and study of tissues. Microscopes will be provided for those students who cannot provide themselves with instruments.

The following general course is open to each student, and is under the direction of Professor Conn. It will consist primarily of laboratory study of specimens illustrating the types of animal life. The practical work will be accompanied by lectures giving an outline of systematic zoölogy, for the purpose of showing the relations of the forms studied to other animals. The lectures will also touch upon various matters of general biological interest. The types studied in course will be as follows: *Protozoa*, — study of microscopic forms, including directions in the use of the microscope; 1. *Cœlenterata*, — hydroids, including the study of jelly fishes and the development of hydroids; 2. *Echinodermata*, — the star-fish; 3. *Bryozoa*, — study of an adult Bryozoan; 4. *Mollusca*, — the clam, the snail, development of the oyster or some other type; 5. *Crustacea*, — the crab, with a study of its development; 6. *Insecta*, — the grasshopper; 7. *Vertebrata*, — dissection of the fish, dissection of the frog.

Accompanying this course of laboratory work and lectures will be given instruction in methods of mounting objects and in the preparation of microscopic sections. Opportunity will also be given for collecting and surface skimming.

A special feature of the laboratory this season will be an extended course in the methods of bacteriological research. The

course will consist of laboratory work on the culture and propagation of bacteria, identification of species, and of lectures and demonstrations by the director. Only those who are well prepared by previous study and experience in biological or medical work will be admitted to the course.

Students who pursue the general course of instruction during the summer, and who have time for extra work, are given the instruction and facilities necessary to enable them to carry on special investigations; while those students who have already gained the knowledge and experience which is provided by the general course, will be permitted to give their entire time to special work.

The laboratory will open for the season on Tuesday, July 7. The regular session for students will continue from that date until Friday, Aug. 28. The number of students for the season of 1891 is limited to twenty-five.

A good reference library will be placed at the service of students, and a collection of *algæ* will serve to guide students in marine botany. In addition to the regular lectures given in connection with the laboratory work, evening lectures will occur two or three times a week, illustrated by the aid of a magic lantern. The lantern is provided with a vertical attachment and with large and small cells, in which forms of life may be placed and their structure exhibited on the screen. A microscopic attachment to the lantern will enable lecturers to demonstrate points in minute anatomy, and a large collection of lantern slides of biological subjects will furnish the means for comparison of many allied forms and structures. The evening lectures will be open to the public, and persons interested may secure admission to the entire course.

For further particulars inquire of Professor Franklin W. Hooper, Secretary, Brooklyn Institute, Brooklyn, N.Y., or of Professor Herbert W. Conn, Ph.D., Wesleyan University, Middletown, Conn. Applications for admission as students should be sent to the secretary of the institute.

THE ETIOLOGY OF TETANUS.

IN a late number of the *Annales de l'Institut Pasteur* there appears (from the Bacteriological Laboratory of Val-de-Grâce) a most interesting paper on tetanus by Drs. Vaillard and Vincent, an abstract of which is printed in a recent issue of the *Lancet*. This paper appears to throw very considerable light on the subject of tetanus, and to clear up a number of points and observations that have hitherto been enshrouded in obscurity. After describing the organism, and identifying it with that already made familiar through the papers of recent writers, the authors give it as their firm opinion that in cases of artificial inoculation of pure cultures it is always the poison introduced along with the bacillus, and not the organism itself, that acts upon the animal. This indeed seems to be probable, as they are able to prove that almost inconceivably minute doses of this poison, which they compare with snake poison, are quite sufficient to produce all the symptoms of most acute tetanus; in fact, it was almost impossible, from some of the cultures that they obtained, to administer a dose that was not lethal.

An exceedingly interesting feature brought out in the course of their work is that in no case was the poison developed as soon as the organism began to grow; in fact, gelatine cultures of the tetanus bacillus were never capable of producing toxic symptoms until liquefaction of the gelatine had commenced, when spores were demonstrated to have been formed, and when the peculiarly disagreeable odor so characteristic of tetanus cultures had become perceptible. They associate both the odor and the peptonizing power with the formation of the poison in the cultures. That it was not due merely to the presence of the spores that the material was poisonous they demonstrated by heating their cultures to a temperature of 62° C., for a short time (a temperature which is quite incapable of interfering with the vitality of the spores), when it was found that cultures so heated and introduced by inoculation into a rabbit or a guinea-pig failed to produce any tetanus, thus proving that, although the spores are not killed, the poison has been destroyed by the heat. The spores were proved to be living by making fresh cultures from them in artificial media; after a time they grew luxuriantly, and if left to grow eight or ten days produced another crop of the poison. By simply washing away the poison from the spores with distilled water they also obtained similar results, for, although the spores could still develop and form the specific poison in artificial media, they were, when inoculated, incapable of giving rise to any symptoms of tetanus. From the re-action to heat of a substance they were able to separate, and from its resemblance to the diastases in other respects, they conclude that they have obtained from tetanus cultures the true tetanus poison, a poison, however, that cannot be formed by the tetanus bacillus in healthy tissues. The micro-organisms are here so rapidly attacked by the leucocytes that they are rendered *hors de combat* before they have time to form their poison.

It has long been well known that the tetanus bacillus could not develop in the tissues except, apparently, in the presence of other organisms, and the suggestion is offered that these other organisms act in one of two ways; they either paralyze the activity of the leucocytes, or they draw off, as it were, their attention and activity from the tetanus bacillus, thus allowing it sufficient time to develop its characteristic products.

It is interesting to note that Drs. Vaillard and Vincent consider that in many respects the tetanus bacillus is extremely like the diphtheria bacillus, the method of action on and in the organism being essentially the same in the two cases, the above factors in all probability playing a part in diphtheria much as in the case of tetanus; and it is evident that in studying the one poison much light may be thrown on the other. Behring and Kitasato appreciated this fact, and combined their forces to work out the question of immunity in these two diseases. It is obvious, however, from a consideration of some of the points that are indicated in this paper, that there are many sources of fallacy that will have to be eliminated before the ultimate explanation of the condition of immunity in protected animals can be given.

The facts that this poison is active in such extraordinary minute quantities, and that the micro-organisms are able to grow with such difficulty in the human tissues, allow us to hope that extremely minute changes in the blood may be quite sufficient to secure the alteration or breaking-down of the virulent poison, even when it has become diffused throughout the system. So long as the organism is localized to the wound, there is, of course, more chance of coping successfully with the disease, although here, as in other diseases, there always appears to be a possibility of the poison exerting such a paralyzing influence on the cells that usually take up foreign substances, that secondary septic conditions may be liable to occur even when the action of the tetanic poison can be antagonized so far as its primary effects on the cells are concerned.

One question appears to be set at rest, and that is, as regards tetanus and diphtheria, the ptomaines have had their day, whatever may become of the products of other organisms. It may be accepted that here, at any rate, we have some subtle poison which, although it has not yet been actually separated, has become so far isolated that it may be taken as proved that it is not an alkaloid or basic poison.

A most remarkable feature is that, in peptonizing gelatine with the filtrate from a meat-broth culture of the tetanus bacillus, the poisonous properties are lost to a certain degree in direct proportion to the amount of gelatine that is peptonized; this, taken in conjunction with the fact that the properties are not developed until the gelatine begins to liquefy, has led Drs. Vaillard and Vincent to suppose that the same agent that peptonizes the gelatine is the active agent in bringing about the development of the toxic symptoms of tetanus.

ONE of the many important uses to which electric welding machines are put is welding railroad rails. Owing to the difficulty of maintaining rails in crowded and paved city streets, it is an advantage to have the rails as long as possible, thereby reducing the number of joints to be cared for, and during the past year a company in Johnstown, N. Y., has been successfully experimenting in electrically welding rails up to 110 pounds per yard. This company is now having constructed one of the largest machines ever built for the purpose. As a result of careful tests, it is claimed that a saving of at least thirty-four per cent is effected by the electric welding process as compared with the older method.

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STEPS are being taken to celebrate the seventieth birthday of Professor von Helmholtz, which occurs on Aug. 31. A marble bust of Professor Helmholtz is being made, which will be presented to him on that occasion, and a fund is being raised, the income of which is to be applied, primarily, to the bestowal of a Helmholtz medal on eminent investigators of all nations in the fields of Professor Helmholtz's activity. An international committee, which has been formed to carry out these schemes, solicits contributions, which may be sent to the committee's bankers, Mendelsohn & Co., Berlin. Professor Henry P. Bowditch of Harvard University will forward the contributions of such as may find it more convenient to send to him, with the names of the contributors, to the bankers appointed by the committee. All contributions should be sent as soon as possible.

JULIUS ERASMUS HILGARD.

MR. HILGARD, whose death on May 8 has been announced, was born at Zweibrücken, in Rhenish Bavaria, Jan. 27, 1825. His father was a man of a wide range of accomplishments, — counsellor at law, judge, poet, classical scholar, and author. Being of liberal tendencies in politics, he became dissatisfied with the *régime* under which he lived, emigrated in 1835, and settled in Illinois, where he personally directed the education of his children. The subject of the present notice also studied in Philadelphia, where he made the acquaintance of Professor Bache. In 1845 he obtained an appointment in the Coast Survey, and soon became one of Bache's most trusted assistants.

His administrative and business tact led to his promotion in 1862 to the position of assistant in charge of the Coast-Survey Office. He now took a prominent part in directing the scientific work of the survey, especially in its relation to the International Metrical and Geodetic Commissions, having their headquarters in Paris. Perhaps his most noteworthy work was that done in connection with the determination of the transatlantic longitude in 1872. Soon after the Atlantic cables were put into successful operation, the difference of longitude between Greenwich and the Harvard College Observatory was determined by Dr. B. A. Gould. Shortly afterward the French cable was laid between Brest and St. Pierre, and it was judged expedient to repeat the determination by taking Paris as the starting-point. It happened, however, that the telegraphic determination of the longitude of Paris from Greenwich, made in 1853, was very doubtful, and it became a necessary part of Mr. Hilgard's work to repeat this determination. This he did with the assistance of Mr. Frank Blake, then assistant on the survey, who observed both at Greenwich and Paris. The result was an important correction to the longitude of Paris, and hence to other European longitudes which depended upon it.

On each occasion of a vacancy in the superintendency of the Coast Survey, Mr. Hilgard was naturally a prominent candidate for the succession. He was, however, disappointed in his aspirations, both on the death of Professor Bache in 1867, and on the resignation of Professor Peirce in 1874. On the death of Capt. Patterson in 1881, his long and efficient service as assistant in charge of the office, and his intimate acquaintance with all the details of the work, made his appointment seem especially fitting; and he was selected for the position with the general concurrence of all parties interested. He had not been long in office before the symptoms of the insidious disease which finally carried him off increased to such a degree that he was obliged to resign in 1886.

Whatever weakness may have been developed in the last years of his life, there can be no two opinions upon the character and value of his life-work in connection with the Coast Survey. He brought into that branch of the public service a rare combination of culture, zeal, knowledge of the world, and executive ability; and no man living will claim to have done more than he did for the character and efficiency of the survey.

THE FERMENTATIONS OF MILK AND THEIR PREVENTION.[1]

SWEET milk is the foundation of the dairy interest. All dairy products are dependent upon milk, and furthermore, they are dependent upon sweet milk, for after it has undergone any of its fermentative changes it becomes worthless either to be used as milk or in the manufacture of butter or cheese. When milk first comes to our hands from the cow it is always sweet, and it has no tendency to undergo any troublesome changes. But this condition lasts only a short time, and sooner or later some form of decomposition begins, and the milk becomes useless. It is our purpose, this afternoon, to study some of these fermentations and to determine if possible some of the facts regarding their prevention. It may be well to say at the beginning that I have no royal road to recommend for the prevention of milk fermentations, since no practical method of preventing them has yet been discovered. But a knowledge of the nature of these troublesome changes and of their causes will go far toward enabling each one to guide himself in avoiding them.

I shall consider the subject under three heads: 1. What are the fermentations of milk? 2. What are the causes of these fermentations? 3. How may the fermentations be prevented?

First, then, we will consider what are these fermentations. We may notice at the outset that they are widely varied. They are by no means confined to the ordinary souring and the fermentation produced by rennet, although these are the only ones that are so well known as to have received special names in the dairy. Every-one, however, who has had any extended dealings with milk, has noticed that it sometimes undergoes changes that are quite different from the normal ones, but which may be none the less troublesome. The various fermentations which are now known to be common to milk have only been recognized within a few years. While the souring of milk has been known for centuries, and the fermentation of milk by the action of rennet has also been long understood, milk has been studied scientifically only about fifty years. During the last fifty years various sorts of decomposition changes have been recognized, one after another, until to-day the number known is quite large. Let us, then, in introduction to our subject, review briefly the most common forms of fermentation which are liable to occur in milk, taking them partly in the order of the commonness of their occurrence.

First, we may notice the ordinary souring of milk, though it is too well known to demand description. This effect is connected with the milk sugar present in the milk. The milk sugar undergoes a decomposition and forms lactic acid, the acid thus formed

[1] An address by Professor H. W. Conn, in December, 1890, before the Connecticut State Board of Agriculture.

rendering the milk sour to the taste and precipitating the caseine in the form of the curd.

Hardly less familiar to you all is the fermentation produced by the action of rennet. You will all recall this action produced by the addition to the milk of a little rennet which you have obtained from a calf's stomach. The milk curdles quickly, and after a little a whey separates from the curd. In this case the action is quite different from that of the souring. It is entirely independent of the milk sugar, and is connected with the caseine of the milk. The caseine undergoes a chemical change under the influence of the rennet. In common sweet milk the caseine is in a condition of partial solution, and while it is in solution the milk is of course a liquid. But under the influence of the rennet a chemical change takes place, the nature of which we do not yet fully understand. So far as we can determine to-day, the change consists of a separation of the caseine into two parts, one of which is soluble, and therefore remains in solution in the whey, while the other is insoluble, and as soon as it is formed it is immediately precipitated as the curd. While, then, the souring of milk concerns the milk sugar alone, the fermentation by rennet is connected only with the caseine.

A third form of milk fermentation is the alcoholic fermentation. Milk does not readily undergo the alcoholic fermentation. When yeast is added to a solution of ordinary cane sugar it causes the sugar to be decomposed into alcohol and carbonic acid. If yeast is put into milk, however, instead of undergoing an alcoholic fermentation, it will under ordinary conditions undergo a change into lactic acid, and will consequently sour. Nevertheless, an alcoholic fermentation of milk does sometimes occur. The Arabs, wandering around the deserts, have been for a long time accustomed to prepare from the milk of their mares an intoxicating drink which contains considerable alcohol. This drink they call "koumiss." It is prepared by simply putting the milk into flasks, and adding to it a little already fermented milk, which starts the process anew, and soon gives rise to a considerable amount of alcohol. In the Caucasus Mountains it has somewhat recently been noticed that the common people have a method of preparing an alcoholic drink from ordinary cows' milk. The milk is placed in leather flasks, and there is added to it some small lumps called "kephir grains." These kephir grains contain various yeasts and bacteria, and they are possessions of the common people, who hand them down from generation to generation. Where they originally came from is unknown. They have the power of setting up fermentation in the milk, at first the ordinary lactic fermentation, but this is soon superseded by the formation of alcohol, and on the second day the milk is in condition to drink. Since it has been found that milk can be made to undergo an alcoholic fermentation, a simple method has been discovered of producing it at will from cows' milk. All that is necessary to do is to add to the milk a little ordinary cane sugar and then a little yeast, and the fermentation that takes place will produce alcohol, and give us a beverage to which the Arab name "koumiss" is applied. This condition of milk is frequently prescribed as a food in hospitals, since it seems to be more easily digested than ordinary milk, the caseine being coagulated into small flakes that are readily acted on by the digestive juices.

The next fermentation that we will notice is that producing bitter milk. All of you must be familiar with this peculiar trouble. At certain seasons of the year, especially in the fall, milk seems to have a tendency to become extremely bitter without becoming sour. Quite naturally, this has been ascribed to some special food which the animals get hold of at this season. It is, however, a troublesome matter, for it spoils the milk and injures it for all dairy purposes.

A fermentation, not quite so common, but far more troublesome when it occurs, is that known as slimy milk. Perhaps some of you have had experience with this milk, that can be more readily sold by the yard than by the quart or gallon. The milk, after milking, rapidly becomes viscous, thickening to such an extent that the vessel in which it is placed may be inverted without spilling the milk. So slimy does it become that it can sometimes be pulled out into long threads, like molasses candy. Such milk is of course worthless. It cannot be churned, the cream will not rise on it, and it is useless for cheese-making. Of course no one wants to drink it. Up in Norway, however, the people are said to be fond of drinking, or rather eating, this slimy milk, and have learned to prepare it artificially by putting a small plant into the milk. With us, however, it is nothing but a troublesome nuisance, and the farmer who finds it in his milk usually tries every imaginable remedy to check it.

Milk not infrequently undergoes a change by which it becomes rancid. It has the smell of rancid butter, and chemical study has shown that the trouble is due to the formation of the same material which gives the taste to the rancid butter, viz., butyric acid. Such a fermentation, though very common, is not ordinarily seen in the dairy, since it is concealed by other more prominent changes, and thus escapes notice.

One of the commonest fermentations of milk is what we may call that of alkaline curdling. Under its influence the milk curdles without becoming acid. I am sometimes asked why milk sometimes becomes "loppered" without losing its sweet taste. It is due to the effect of the fermentation that we are now considering. Such a curdling seems to be similar to that produced by the action of rennet. Indeed, careful study seems to indicate that the two are almost, if not precisely, identical, and that these alkaline fermentations are produced by the formation of a ferment similar to rennet. This form of fermentation represents a class of which there are many varieties. They are accompanied by various odors and smells, and the milk seems to be undergoing decomposition. The various forms of tainted milk may be usually ascribed to the class of fermentations now considered. They are certainly very common, almost always occurring in milk which has stood for a short time, but commonly they escape notice, since the souring of the milk is so much more prominent that it entirely conceals the alkaline curdling. Experiment, however, easily isolates this fermentation.

Once in a while dairymen are troubled by a blue milk, not blue milk like that of the city restaurant, which is blue simply because the cream has been removed from it and water added, but milk which is blue from a special fermentation. Such milk appears like other milk when it is drawn, but just about the time it begins to sour, small blue patches may be seen in it. These patches increase in size, and finally, by the time the milk is quite sour, it has assumed a brilliant blue color. No one wants to drink such milk, though it is probable that it would not do any injury if it were drunk. There is no poison in it that chemists can discover, and it has been fed to small animals like rats without doing any injury. But still no one with his eyes open will drink it, and if it is known that the milk from a certain farm is subject to this fermentation it will be thoroughly avoided. Sometimes this blue milk becomes so common that it may almost be regarded as an epidemic.

Blue milk is not the only colored milk that arises as the result of fermentation. Yellow milk sometimes occurs. I have had in my laboratory milk that is just the color of a lemon; other specimens with an amber color. Red milk is occasionally found, sometimes occurring spontaneously as a troublesome infection, and easily produced artificially in the bacteriologist's laboratory. Sometimes milk assumes a green color, though never quite so brilliant as a grass green. Such milks usually have a vile odor, and are plainly undergoing a putrefactive decomposition. A violet milk is also occasionally seen.

Lastly, I may mention a series of fermentations under the head of miscellaneous. Various forms of decomposition changes occur which do not really belong under any of the above classes, and which have not been sufficiently studied to enable us to say much about them. They simply indicate that in the above list we have by no means exhausted the fermentations which are likely to occur in milk, and that future study will reveal much more in this line.

It is only within a comparatively few years that this long list of fermentations has been known. Little by little, as milk has been studied by modern scientific methods, has the number of these known fermentations increased, and nearly every year adds one or more to the list of the fermentations to which milk is known to be subject.

We will now pass on to the second division of our subject, and ask what is the cause of these fermentations. The alcoholic fermentation and that of rennet may now be omitted from discussion, for every one knows that these are produced by adding something to the milk, a yeast in the one case, and rennet in the other. Leaving aside these, then, we would naturally expect, inasmuch as the other fermentations are very varied, to find their causes varied also. In a certain sense this is true, but at the same time there is one point in which they all agree. All of the fermentations mentioned above are due to microscopic plants getting into the milk subsequent to the milking and their growing.

Before passing to a further consideration of this matter, it may be well to notice that there occurs, very rarely, a curdling of milk which is not due to micro-organisms. Once in a while milk is found to curdle almost as soon as it is drawn from the cow, and in this case the trouble is not due to micro-organisms. Such an occurrence is extremely rare, however, and it is doubtful whether any of you have ever had any experience with it. But aside from this rare occurrence, all of the fermentations are caused either by bacteria or yeasts, which get into the milk subsequent to the milking.

It has taken many years to reach this conclusion. It will be convenient for us to consider the fermentations as belonging to two classes, one of which we may call the normal fermentations, and the other the abnormal fermentations. The former class includes only the common souring and the rennet fermentations, while the abnormal class includes all of the others. Now it has been recognized from the very earliest times that the abnormal fermentations were due to something getting into the milk which did not belong there. So long ago as 1838 a microscopic study of blue milk revealed in it some micro-organisms, and these were even then suggested as the cause of the trouble. From that time, as one after another kind of fermented milk was studied, it was seen that they were all associated with some form of bacteria, and the conclusion is now very definitely proved that they are all caused by these organisms. All of the forms of fermentation mentioned above have been associated with definite species of bacteria, and all can be artificially produced by inoculating good milk with the right species of bacteria.

After it was seen that bacteria were the cause of the troubles, the next question was to account for their presence in the milk. It did not seem possible at first that they could all get into the milk after the milking. All sorts of explanations were suggested relating to conditions surrounding the cow. The cow was supposed to have caught cold, or to have been heated, or to have run too fast, or to have been eating some injurious kind of food, and for some of these reasons the milk fermented. Every thing was blamed except the carelessness of the milker. I imagine that many of you even to-day think you have very good reason for believing that certain fermentations are really caused by the food that the cow eats, and this has always been the favorite excuse. You have, perhaps, found slimy milk in your dairy, and have then remembered that recently you begun to feed your cow on a special lot of meadow hay. Thinking that this might have caused the trouble, you ceased to feed this hay and the trouble ceased. What better proof could you desire that it was the hay that the cattle ate which produced the slimy milk? In fact nothing of the sort is proved by this experiment. Do you not remember that when ensilage was first introduced, many farmers complained of it, saying that its use caused their milk to become tainted, and thus much injured its quality? And do you not also remember that as experience began to accumulate it soon appeared that it was not the ensilage which the cows ate which produced the trouble but the ensilage which the milker handled? To-day you know that you can feed ensilage to the cow with no danger provided that you exercise sufficient care in handling it, and allow no opportunity to occur for the ensilage to contaminate the milk after the milking. So it is with all other ferments. It is not the food that the cow eats that produces the fermentation, but it may be the food that is in the barn, and is being constantly stirred up so as to keep the air full of floating bacteria. These may get into the milk and produce trouble, and they will be avoided by letting the hay alone or doing the milking out of the proximity of this troublesome food. The cow may eat it with impunity. The remedy is not to change the food but the conditions of the milking-yard and the dairy.

Do not understand that I would infer that the food the cows eat has no influence on the taste of the milk. There is no question that if the cows eat a strong-tasting food like garlic, the taste is transferred to the milk. But this is a very different thing from the production of fermentation. The taste produced by such food is at its maximum as soon as the milk is drawn, while in the case of a fermentation the effect is an increasing one, being absent at first, but appearing as the bacteria have chance to grow.

While thus it is seen that the unusual fermentations have long been ascribed to the action of bacteria or something else getting into the milk which does not belong there, this has by no means always been supposed to be true of the common souring of milk. The souring is a universal and not an occasional thing, and there seemed for a long time to be no way to prevent it. So long ago as 1844 bacteria were found in souring milk, and it was even then suggested that the souring was due to them. In 1850 again the fact was reaffirmed. Pasteur commenced his work on milk about 1860, and finding that he could prevent the souring by subjecting the milk to a high heat, and, moreover, being always able to discover in it numerous bacteria, he insisted that even this common fermentation was due to these organisms. The statement did not go unchallenged, however, and for the next ten years there were conflicting results. In 1874, and later, Lister and Hall succeeded in procuring milk directly from the cow with such precautions as to avoid chance of contamination by bacteria, and they found that such milk remained sweet indefinitely without showing any tendency to undergo even the souring fermentation. After this there could no longer be any question in regard to the matter, and we may therefore ascribe the souring of milk to the same class of causes as those producing the more unusual fermentations.

It may seem somewhat remarkable that bacteria should so universally get into milk. But the fact is that they are very abundant everywhere. They are in the air, in the milk vessels, on the hands of the milker, on the hairs of the cow, and above all they will be inside of the milk duct, extending for a short distance from its mouth. Some milk will always be left in the mouth of the duct, and in this milk the bacteria will grow and remain there ready to contaminate the next milk that comes out. The number of bacteria in milk is very great, and I can hardly believe the figures which are indicated by my own experiments. I have found in milk which has been only two or three hours drawn from the cow as many as 20,000 to 40,000 to each teaspoonful of milk. These numbers are surprising, but they are not so large as have been found by certain German experimenters. In milk that has been standing for a little while they increase wonderfully, so that by the time the milk reaches the city their number is prodigious. I suppose no one in a city ever gets milk to drink that contains a smaller number of bacteria to the teaspoonful than there are inhabitants in the United States according to the last census.

We are now ready to pass to the third head of the subject, the prevention of the fermentations. As I stated at the outset, I have no royal prevention to recommend for this, and can hope only to throw out some suggestions which each may apply to its own special troubles. We may set aside the fermentations produced by rennet, and the alcoholic fermentations, because these are always produced by adding something to the milk, and may therefore be easily prevented.

Now, if all other fermentations are due to the growth of bacteria, we have only to keep them out of the milk in order to prevent them. This is, however, entirely impracticable. The bacteria are so abundant, and they lurk in so many places, that no practical method can be adopted to prevent them from getting into the milk. Especially is this true of the souring species. We find that the souring of milk is produced by a number of species of bacteria, and these are marvellously numerous about the barn, and more particularly in the dairy. Perhaps care may lessen their number, but it cannot do away with them altogether.

This is not true, however, with regard to the bacteria which produce what I have called the abnormal or unusual fermentations. The bacteria which produce slimy milk, bitter milk, blue milk,

etc., are not common in the dairy, and they may be kept out of the milk by using sufficient care. Their home is in filth, and they are usually troublesome because of uncleanness. Go to an ordinary cow shed and look at the conditions surrounding the cows. The cows are usually covered with filth, and are practically never clean. They wander at will through the day in swamps, or any muck that they may happen to find, and have every facility for dragging their teats through the mire, or they lie in the mud, and thus insure the fouling of the bag and teats. At night they have no choice except to lie in filth. The farmer goes to the milking in a condition frequently almost as dirty as the cow, and uses vessels which are never thoroughly cleaned. What better chance could there be for filth bacteria to grow? If there are any troublesome bacteria around, they will be sure to get into the milk in some way, and the wonder is not that milk sometimes undergoes troublesome fermentations, but that we ever succeed in getting milk fit to drink. These are usually the causes of the troubles that the farmer has with his milk.

I have in mind now a cheese factory which was at a certain season troubled with a badly-tainted milk, and this finally became so troublesome as to interfere with its business. A man with a pair of bright eyes was set to work to discover the cause of the trouble. He soon succeeded in tracing it to the milk of a single customer. If the milk brought by this man were kept away, the rest remained all right. Examining into the conditions of this patron's farm, he found that the cows were in the habit of wandering through a slimy swamp, and that the material from the swamp would get into their hair and teats, and hence in the milk. This was the sole cause of the trouble, for as soon as the cause was removed, the milk was good again.

I repeat, then, that the abnormal fermentations of milk can be prevented by using sufficient care. The time is coming when the farmer will be ashamed to own that he is troubled with slimy or bitter milk, for it will be regarded as indicating a lack of sufficient care and cleanliness in the arrangements of his barn. Keep your cow sheds clean, clean the cows themselves, give them clean beds to lie on, wash their teats, sand the floor, let a little of the first milk that runs out of the teat fall to the floor instead of into the milk vessel. If you want to convince yourself of the value of this last procedure, try the experiment of letting the first milk run into a separate vessel, and then see how much sooner it will sour than the rest. The first milk that comes out partly washes the milk duct, and hence contains the bacteria in great numbers. Clean your hands before you milk, and, above all, exercise more care in cleaning the vessels in which you milk. These cannot be sufficiently cleaned by a simple short scalding with hot water. Boil them once in a while for a long time on the stove, and you will find the time well spent.

These, then, are the remedies for all of the unusual fermentations, and every one must apply them for himself. It is impossible to tell beforehand where the trouble lies in your special case. It may be in the condition of the cow, or in the condition of the food, or the milker, or in the dairy itself; but, if you only look carefully for it, you will always find the mischief lies somewhere, and can be avoided by the exercise of sufficient care.

It is as important to make a careful toilet for the milking shed as for the supper table. Indeed, is it not more so? At the table a little dirt will produce no special trouble, but in the milking yard it may entail much trouble on yourself, and all using your milk in any form.

All of this will not, however, prevent the ordinary souring of milk. In spite of the greatest care, the bacteria which cause the lactic fermentation will get into the milk, and there is no practical way of avoiding them. Is there, then, any way by which the souring of milk may be prevented?

We may first ask if we cannot kill the bacteria after they get into the milk, for if this can be done, of course the milk will not sour. The simplest suggestion is to find some chemical which will kill them. It is easy enough to find such a chemical. Corrosive sublimate will poison them, and will also poison any one who may subsequently drink the milk. Of course such a violent poison will not answer. It is necessary to find something that will poison the bacteria and at the same time be harmless to man. One of the first substances ever used for this purpose was horse-radish. More than fifty years ago it was stated that horse-radish would prevent milk from souring. But when we drink milk we want it to taste like milk, and not like horse-radish. The poison used for preserving milk must, then, not give a taste of its own to the milk.

Within the last few years several chemicals have been tried for this purpose with some little success. Those most used are carbonate of soda, borax, boracic acid, salicylic acid, quick-lime, and some others not so common. In regard to these, we may summarize the results of recent experiments briefly as follows: Salicylic acid is of the most use in delaying the souring of milk. It can be used in proportions of 1–1000, about a teaspoonful to a gallon of milk. Borax comes next in value. It may be used in proportions of 3–1000, about three spoonfuls to a gallon. When used in these proportions, the two preservatives mentioned will assist the milk in keeping sweet for a short time longer than if they were not used. None of the others seem to be of any value, or at least of not enough to make it worth while to use them. Most of the preservatives sold in the market to-day are some compounds of these chemicals, and it is just as well for the farmer to buy the borax or salicylic acid pure, as to buy the patent mixture, and pay the price of the patent. At best, however, the use of chemicals for preserving milk is very limited, and it is not recommended to-day by any who have made a study of the fermentation of milk.

The method of milk preservation most commonly in use is that of heat. It is well known that high heat will kill all living things, and, of course, if milk be heated hot enough, the bacteria in it will be destroyed. It is found, however, that a temperature of boiling is not sufficient to kill all of the bacteria in milk. The bacteria in milk are in two different conditions. Some of them are active, perhaps swimming around in the milk, and are always rapidly growing. Others are in a dormant condition, which is known as a condition of spores. The spores correspond in a measure to seeds, and although they are dormant, each one has in itself the power to germinate and produce anew the active form of bacteria. Now it is found, that, while the temperature of boiling will kill all of the active forms, it will not kill the spores. To kill these by heat, the milk must be heated under pressure, since this renders it possible to obtain a higher temperature. A temperature of 230° F. will destroy these spores, and render the milk absolutely without life, absolutely sterile. Such milk will keep indefinitely without souring or undergoing other fermentation.

Of course it is not an easy matter to heat milk under pressure, and some other method of accomplishing the same purpose is desirable. It is found that a long continued boiling at the ordinary pressure of the air will sterilize the milk. It is also found that sterilization may be accomplished by what is called discontinuous heating. This is simply heating the milk to a temperature of boiling for a short time on several successive days. If milk be placed in a bottle and boiled a few minutes upon three successive days, it will be sterilized and remain subsequently without bacteria growth.

Based upon these facts regarding sterilization, a large number of forms of apparatus have been invented for conveniently accomplishing the heating. Several sterilizers of milk are on our markets, and still others in Europe. One of the simplest methods of sterilization is within the reach of every one. Place some milk in bottles with long necks and plug the neck with a wad of cotton wool. Then place the bottles in a common steamer, with which almost every house is provided, and steam the milk for an hour. This may not absolutely sterilize the milk, for a very few bacteria in the form of spores may be left alive. But it will so nearly accomplish the purpose that the milk will keep perfectly sweet for many days, and may be carried on a journey with impunity, provided the cotton plug is not removed. If desirable, a common cork can be put in the bottle on top of the cotton plug, to prevent the spilling of the milk.

The use of sterilized milk is rapidly becoming common. A few years ago no one ever heard of it, but now, especially in the cities, where it is impossible to get fresh milk, its use is growing rapidly. In the case of sickness affecting the digestive organs, doctors are

learning to recommend that all milk should be sterilized. Indeed, doctors have for a long time been accustomed to recommend boiled milk to patients, but formerly from a mistaken idea. It was always supposed that boiling the milk rendered it more digestible, just as cooking other food makes it more easy to digest. Within recent times, however, we have learned that boiled milk is not more easily digested than fresh milk, but, on the contrary, that it is far less easily digested. If an animal is fed with a certain quantity of boiled milk, and subsequently with an equal quantity of fresh milk, he will digest and absorb only about two-thirds as much of the boiled milk as of the fresh milk. The reason that boiled milk is better than unboiled milk for invalids is because of the presence of bacteria in the latter. In our cities, as we have seen, these are extremely abundant in all milk; and although to the ordinary healthy person they are harmless, they may be a source of irritation to one whose digestive organs are out of order, and therefore in an irritable condition. It is believed that nearly all of the cases of cholera infantum in our cities are due to the bacteria present in the milk drunk by infants. Nursing children are much less liable to have the disease, since they obtain their milk fresh and free from bacteria. It is not surprising that the doctors in our cities are learning that one of the first things to do in the case of intestinal diseases is to prevent the patient from taking in the large quantities of bacteria which he would swallow with unsterilized milk. I know of one doctor who goes further, and furnishes his patients with sterilized milk in order that he may be sure they obtain it.

There are two disadvantages in sterilizing milk by boiling. The first is that the milk is not thereby completely sterilized, and is likely to undergo some fermentation after a time. This is not a very serious matter, however, for the milk thus sterilized is pretty sure to be used before any of these fermentations occur. Milk that is sterilized is not usually intended for long preservation, but for using immediately, or, at least, within a few days. This being the case, it is not a matter of much importance if some of the spores of the resisting bacteria should be left in it in condition to set up a fermentation after a week or more.

The other disadvantage is a more serious one. The milk thus sterilized has not the taste of fresh milk. Every one is acquainted with the taste of boiled milk, and we all know that it is not so pleasant as that of fresh milk. To some it is quite disagreeable, and children frequently will not touch it. Now, any sort of sterilization by boiling is sure to cause the milk to acquire this taste of boiled milk. This taste appears at about the temperature of 160° F., and, since all methods of sterilization by heat raise the temperature much above that point, the taste of boiled milk is always found accompanying such sterilization.

Now, there is a method of sterilizing milk which avoids the production of this taste, but it is long and tedious. If the milk be heated to a temperature of 155° F. for twenty minutes upon six successive days it is commonly found to be sterilized, and, since it has not been heated to 160°, its original taste will be preserved. Such a process is, of course, too long to be of any practical value, except for scientific experiment.

The fact is, that with our present knowledge, there has been devised no way of sterilizing milk without either producing the disagreeable taste of boiled milk, or being so long about the process as to render it of no value in practice.

It is, however, possible to produce, with ease, a partial sterilization. It is frequently of great value to one dealing with milk to delay the souring as long as possible, and if this fermentation can be put off for a few hours even, it may prove of great use. There has been invented in Paris a method of treating milk which accomplishes just this. It is known by the name of pasteurization. It consists simply in heating the milk for a few minutes to a temperature of about 155°, or a little higher, and then rapidly cooling it. The short heating does not indeed kill all the bacteria that are in the milk, but it does very much diminish their numbers. So much does this heating check the bacteria growth that it is found to delay the fermentation of milk from twenty-four to forty hours. Of course such a delay as this is of the greatest value in our cities. For accomplishing this pasteurization several machines have been invented, all of which enable a large amount of milk to be heated in a short time. In some the milk is caused to run over metal plates that are kept hot by steam; in others the milk is in a large vessel and the steam conducted into the vessel in a coil of pipes. All of them accomplish the same purpose, but not with equal facility.

There is one advantage arising from pasteurization which renders its practice even more valuable. It is found that nearly all, if not quite all, of the pathogenic disease germs which are likely to occur in milk, are killed by the pasteurization. It is well recognized to-day that some of our dangerous epidemics are transmitted from house to house by means of milk. Milk furnishes a good medium for their growth, and has every chance of becoming contaminated. In cities epidemics of typhoid have been repeatedly traced to the milk supply. Now, if pasteurization is sufficient to kill these disease germs, and if at the same time it delays the souring from twenty to forty hours, and if the milk thus treated retains the taste of fresh milk, and permits the cream to rise on it in the natural way, it is plain that pasteurization is a process which is highly to be recommended. It is not surprising that in Paris, and in some of the large cities of France and Germany, pasteurization of milk is becoming more and more common. In Paris it is a regular business, and pasteurized milk is sold at a trifle advance over the price of ordinary milk. People are beginning to prefer it, since it keeps so much better, and is so much safer, and withal has all of the good qualities of fresh milk. It has been suggested that pasteurization of milk in cities should be required by law. So far as I am aware the pasteurization of milk has not yet been introduced into America.

Lastly, a word in regard to the value of cold in delaying fermentation. Every one knows that milk will keep longer if it is kept cool, and it can be preserved almost indefinitely when frozen. But every one is not aware of the great value of a temporary cooling of milk. When milk is drawn from the cow it is at a high temperature, and is, indeed, at just the temperature at which the bacteria will grow the best. The bacteria which get into the milk during the milking, therefore, begin immediately to multiply with great rapidity. If, however, the milk be cooled to as low a temperature as possible, it will take several hours' exposure to the ordinary temperature of the air to bring it back again to the condition where the bacteria will grow so rapidly. Indeed, except in the very hottest summer weather, it will not again become so warm as when it left the cow, and hence will not again offer such a good chance for bacteria growth. It follows, then, that a cooling of the milk immediately after milking is of the greatest possible value in enhancing its keeping properties. Milkmen should remember that half an hour's cooling of the milk, or even less than that, immediately after milking, will save several hours in the souring time, and in hot summer weather this fact should be remembered as one of the best methods of assisting in supplying customers with good milk.

Allow me now to summarize the important points which have attracted our attention this afternoon:

1. The fermentations of milk are varied, although only a few are commonly recognized because the souring of milk usually obscures all other fermentations.

2. All of the fermentations except the fermentations of rennet are caused by micro-organisms getting into the milk after milking and growing there.

3. The micro-organisms are so abundant around the barn and dairy that they cannot be kept out of the milk by any amount of care.

4. The bacteria which produce the abnormal or unusual fermentations, like slimy milk, bitter milk, etc., are, however, not so common but that they may be prevented from entering the milk in sufficient quantities to produce serious trouble.

5. Filth is ordinarily their source, and cleanliness the means of avoiding them.

6. The souring of milk cannot be prevented even by the greatest cleanliness.

7. Salicylic acid in proportions of 1–1000 may be of some little value in delaying the souring, but its use is not to be recommended except in special cases.

8. Milk can be entirely deprived of bacteria by the exposure to

a temperature of fifteen to twenty degrees above boiling water, or by a long-continued boiling, or by a series of short boilings on successive days.

9. Such milk has the taste of boiled milk. This taste appears at about the temperature of 160° F. Hence has arisen the method of pasteurization of milk. By this method it is heated to a temperature of 155° F. for a short time, and then cooled. This greatly delays the fermentations, and also kills the pathogenic germs that may be present.

10. In our large cities the popularity of sterilized milk is rapidly increasing, especially in the case of milk given to patients troubled with diseases of the digestive organs.

11. A cooling of milk immediately after it is drawn from the cow is of the greatest assistance in delaying the fermentation, and is probably the most practical method which can be recommended according to the present state of our knowledge.

HEALTH MATTERS.

Sneezing One's Teeth Out.

THE report of the physician in charge of the Ningpo Missionary Hospital for the past year, says the *British and Colonial Druggist*, contains some interesting observations on tooth-drawing in China. Dr. Daly remarks that Chinese teeth are much more easily extracted than those of Europeans. The native dentists are said to possess a wonderful powder, which is rubbed on the gum over the affected tooth. After an interval of about five minutes the patient is told to sneeze, whereupon the tooth falls out. Dr. Daly has offered a reward of $100 to any one performing the operation in this way in his presence, on condition that he is allowed to choose the tooth and examine the mouth before and afterward. So far no one will consent to perform the operation on these conditions.

Alcohol and Digestion.

From experiments made on himself by Dr. Eichenberg, says the *Medical and Surgical Reporter*, some further knowledge of the effect of alcohol on digestion is obtained, which contrasts strongly with the teetotal lecturer's experiment showing how digestion in a glass vessel is retarded by alcohol. Dr. Eichenberg found that a small dose of strong alcohol — e.g., brandy — shortens the time that food in general, whether animal or vegetable, or a mixture, remains in the stomach by more than half an hour. A similar but not quite so marked an effect is produced by a dose of diluted hydrochloric acid or mustard. Pepper and condurango diminish the time the food remains in the stomach by about a quarter of an hour. Beer and an infusion of rhubarb had no effect.

LETTERS TO THE EDITOR.

⁎⁎⁎ Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

The editor will be glad to publish any queries consonant with the character of the journal.

On request, twenty copies of the number containing his communication will be furnished free to any correspondent.

American and European Meteorology.

FROM time to time discussions have appeared in foreign journals comparing weather conditions and laws of storms in Europe with those in America. These have often shown a remarkable difference between the results announced abroad and those found in this country, and it has been a matter of great difficulty to determine the exact cause of the discrepancies. In the matter of the recent animated discussion as to the temperature at some height in the atmosphere in high areas and storms, it has been suggested already that most of the differences are due to the fact that in Europe the ordinary paths of storms are far to the north-west, over Iceland; and in consequence none of the conditions experienced in this country, on the passage of a storm over a mountain, could be studied in the south-east quadrant of storms in Europe (see this journal, June 6, 1890, p. 346). A very interesting illustration of this point has just appeared in *Meteorologische Zeitschrift* for April. Dr. Hann reviews a paper by Professor Russel, "Prediction of Cold-Waves," originally published in the *American Journal of Science* for December, 1890, and closes with the following words:—

"Of the fact, that the principal cause of cold in winter is local heat-radiation at the earth's surface, the author has no foreboding (*Ahnung:* there seems to be no exact English equivalent), which indeed can scarcely be believed, since his own discussion sets it forth with such certainty. This discussion has only a negative value in that it shows how one, in setting up a rational system of weather forecasting, should not go too far in its seeming certainties." It is not my purpose, nor is it necessary, to defend Professor Russel in his position; but Dr. Hann's view is founded on so faulty a process of reasoning from known conditions in Europe to those which are supposed to exist in this country, that it should not be allowed to pass without comment.

I have already given in this journal (Feb. 27, 1891, p. 121) a statement of the conditions accompanying cold-waves in this country, and it seemed wise to make a partial study of cold-waves in Europe. To this end I first selected out all the cases during December, January, February, and March, in the years 1881–89, which showed a fall of 10° C. (18° F.) in twenty-four hours at Vienna, Austria. It should be noted that the cold-wave discussed by Professor Russel was a fall of at least 20° F. in twenty-four hours, and a temperature reaching 36° or below over an area of at least 50,000 square miles. Dr. Hann says he does not understand this 36°, and suggests that it may mean 36° below zero! This is most extraordinary, and shows how extremely deficient is the knowledge on this subject in this case. No cold-wave of this character has occurred in this country in the last ten years. Dr. Hann probably has in mind the cold of a Siberian winter, where temperatures of —70° are often experienced. The following comprise all the temperature-falls of 18° F. at Vienna: (1) Jan. 14, 1881, from 25° F. to 7°; (2) Dec. 29, 1882, from 48° to 30°; (3) Jan. 31, 1884, from 50° to 32°; (4) Feb. 28, 1886, from 26° to 7°; (5) March 3, 1888, from 33° to 15°; (6) Feb. 12, 1889. On examining the weather-maps for these dates, it was very quickly found that there is absolutely no comparison between the temperature-falls in Europe and those in this country. In most of the six cases there was a high area to the south, and almost a calm; the conditions were favorable for radiation from the earth; but in no single case was there a cold-wave. In (4) there was a high area to the north; but here only one other station, out of fifty-eight all over Europe, reported a fall of 18° F. In not one of these cases was there a fall of temperature over a large region, but it was almost entirely confined to single localities in a very large region, and was manifestly due, as Dr. Hann suggests, to radiation from the earth. In this connection it will be an interesting contrast to give a summary of cold-waves in this country found by Professor Russel between the years 1880 and 1889, statistics of which have been published in the "Annual Report of the Chief Signal Officer for 1891." The total number counted is 619, or an average of 62 in each year. Five of these cold-waves had a fall of 20° F., extending over a region more than 1,000,000 square miles in extent, and in eighty-seven cases the same fall occurred over more than 500,000 square miles.

It is well known that our cold-waves are due to the rather rapid passage across the country of a storm which is followed by a high area. Wherever the cold air may come from, only a very small proportion of it is due to heat-radiation, the principal cause suggested by Dr. Hann. It seemed advisable to study the storms and high areas passing over Europe. I took out all the cases in which these conditions were near Sonnblick during all the months 1887–89. There were fourteen storms and twenty-six high areas. Of these, only one storm, on Oct. 22, 1889, had any thing like the characteristics of storms in this country. In all the three years there was not a single high area that was similar to those experienced here. The evidence furnished by this study was most remarkable, and showed that no comparison whatever can be instituted between these conditions and their accompaniments in the two countries.

In 1884 there was established a high-level observatory at Ben Nevis, in Scotland, over 4,000 feet in height. A great deal has been expected from this observatory, lying as it does almost in the pathway of depressions unheard of in any other part of the

world, reaching as low as 27.4 inches. The observations for four years, 1884 to 1887, have just reached this country. During the four years sixty-eight storms and twenty-four high-areas have crossed over or very near the summit. As far as studied, the results have shown very materially different conditions here from those at Mount Washington. This is due in part to the lowness of the mountain, and in part to the proximity of the ocean on the west or on the side from which the storms advance. A comparison between Mount Washington and Ben Nevis shows, if any thing, that temperature and moisture have little or nothing to do with the generation of storms. At Ben Nevis the most extraordinary depressions are accompanied by only the slightest change in temperature, while at Mount Washington most remarkable changes in temperature are accompanied by much smaller changes in pressure. These facts would seem to show the extreme need there is of confining ourselves to the certainties of our own studies and conditions, and also the absolute impossibility of making and comparing any except the very broadest generalizations regarding weather conditions in Europe and America. H. A. HAZEN.

Washington, D.C., May 8.

Flying-Machines.

THE communication from Mr. H. A. Hazen in the issue of *Science* for May 1, and his quotation from Le Conte, already familiar, I presume, to many readers, suggests the following "deadly parallel:" —

(1) We cannot devise a method of utilizing fuel or a source of energy that shall equal the bird (land-animal, or fish).

(2) We can never build a machine which shall be as perfectly adapted to its purpose of self-transportation as the bird (the land-animal or the fish).

(3) There is a limit of weight, say fifty pounds, beyond which the bird cannot fly (one at which the animal cannot run, the fish live and swim).

Ergo, we can never build a flying-machine to carry a man [a railway train to excel the trotter at a mile in two minutes, the whale of a hundred feet length, swimming fifteen miles an hour].

Remembering what the first century of the operation of man's unimpeded inventive power has accomplished, with steam, with electricity, and with the infancy of his machinery, may it not be just as well to cease the attempt to define the impossible? T.

AMONG THE PUBLISHERS.

A QUESTION that has often been discussed is, whether it would be possible to produce rain at will by the use of explosives. It has been claimed by some that rain has followed cannonading, and to test the matter experimentally the latest Congress appropriated

Publications received at Editor's Office, May 4-9.

FISKE, A. K. Beyond the Bourn: Reports of a Traveller returned from "The Undiscovered Country." New York, Fords, Howard, & Hulbert. 222 p. 16°. $1.

FLOWER, W. H., and LYDEKKER, R. An Introduction to the Study of Mammals Living and Extinct. London, Black. 763 p. 8°. (New York, Macmillan, $6.)

HANS ANDERSEN'S Stories. Newly translated. In two parts. Part II. (Riverside Literature Series, No. 50.) Boston and New York, Houghton, Mifflin, & Co. 205 p. 16°. 15 cents.

HORSFORD, E. N. The Defences of Norumbega: A Letter to Judge Daly. Boston and New York, Houghton, Mifflin, & Co. 84 p. f°.

MACFARLANE, A. Principles of the Algebra of Logic. Edinburgh, David Douglas, 1879. 155 p. 12°. (Boston, Ginn, $1.35.)

WATSON, L. H. Not to the Swift. A Tale of Two Continents. New York, Welch, Fracker Company. 399 p. 12°. $1.25.

WESTERN Bookseller and Newsdealer, The. Vol. I. No. 1 *e. o. w.* Chicago, Western Bookseller. 40 p. 8°. $1 per year.

two thousand dollars. Many of the arguments in favor of the scheme can be found in "War and Weather," a book published by the author, Edward Powers, Delavan, Wis. Of the contents, we may mention "Evidence that Artillery Firing sets in operation certain Motions and Forces that cause Condensation of Aqueous Vapor, thus bringing Rain," "Objections and Criticisms Answered," and "Estimate of the Cost of producing a Rain-Storm."

—Macmillan & Co. have published a cheap edition, limited to ten thousand copies, of Mr. Bryce's great work on "The American Commonwealth." This author's edition is complete in every respect. The public is warned against the purchase of unauthorized reprints of this work which are incomplete and omit the American copyright matter, which forms a part of the book absolutely essential to its value and interest.

—*Our Language* "is designed to be a help for those who wish to increase their knowledge of the English language. It wil serve az a channel for conveying information contributed by leading scholars in this field to those eager to receive it, and az a medium of communication through which students of English may obtain one another's aid in deciding questions of etymology, the use of words, spelling, pronunciation, etc.," which is as clear as the statement found in the first number that "Charley Chu, a New York laundryman, haz this line of Celestial English on hiz hand-bilz: 'If eni article not being proper done up, then we wil do them over free by returning them.'"

—From Macmillan & Co. we have received "Elementary Lessons in Heat, Light, and Sound," by D. E. Jones, professor of physics in the University College of Wales. The lessons are intended to serve the purpose of introducing beginners to the study of experimental physics, and besides providing a certain amount of useful information, they also give an idea of the methods by which this information can be most directly obtained. For this reason the course has been made experimental, and such brief instructions are given as are required for making or using the necessary apparatus and carrying out the experiments.